that in addition to the volume of intraluminal thrombus, its location and impact on the geometry of the arterial lumen can affect the wall stress and ultimately result in variable pattern of aneurysm growth. A prospective longitudinal is therefore required to assess the changes in thrombus volume, resultant change in lumen curvature, and pattern of aneurysm growth/expansion, before we can say that tortuosity of lumen centerline is the key determinant of increasing the aneurysm wall stress.

Another important area worth exploring is the material behavior of intraluminal thrombus. It is most likely a non-homogeneous material with a complex property. A large ex vivo experiment is needed for future study in this area to improve our understanding of aneurysm material properties and failure strength. This can help in creating more realistic computational models, which could be used as a clinical adjunct in the future for effective decision making in aneurysm repair.

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Regarding "Risk-adjusted 30-day outcomes of carotid stenting and endarterectomy: Results from the SVS Vascular Registry"

We have read the study by Sidawy et al[1] about 30-day outcomes of carotid artery stenting (CAS) compared with endarterectomy (CEA) with great interest. Unfortunately, we are forced to conclude that major flaws in the study prevent drawing meaningful conclusions from this observational cohort study. Although cohort studies can sometimes be powerful tools in assessing treatment effectiveness,[2] the authors' study fails to meet minimal standards for such studies; most importantly, (1) complete and unbiased follow-up of study end points, and (2) rigorous control for confounders.

The validity of the conclusions drawn from a study such as theirs, with only 44% follow-up, is extremely limited. Those lost to follow-up are likely to be less adherent to concomitant drug therapy and are often more likely to have had complications; or in contrast, sometimes those who are doing extremely well may waive follow-up visits.[3-5] In general, 80% follow-up in longitudinal studies is considered a minimum, and >90% follow-up is generally feasible in short follow-up studies like that of Sidawy et al.[1] This study's poor follow-up is made worse by the different follow-up rates between groups, by the reliance on self-report, and by the presence of systematic differences between CEA and CAS follow-up, because Centers for Medicare and Medicaid Services rules require in-hospital results for CAS for recertification, whereas there is no such mandate for CEA.

Furthermore, controlling for all potential confounders is always important in cohort studies, but particularly in those in which the choice of intervention is heavily influenced by patient factors (ie, "selection").[2] Those who are more frail or who have higher surgical risk are generally much more likely to have a minimally invasive procedure (CAS) instead of a surgery (CEA), and biases due to such patient and provider selection are notoriously difficult to adjust for in cohort studies, usually requiring special methods such as instrumental variable analyses.[2]

The results of Sidawy et al are in conflict with several randomized controlled trials comparing these two interventions and reporting equivalence of the two interventions,[6-9] even in the long-term.[6] Therefore, we suggest that the short-term differences found in this observational cohort study with poor follow-up and likely inadequate control for confounding do not provide useful evidence on this important clinical topic.

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Reply

The authors appreciate the comments from Drs Meier and Hayward indicating potential flaws in our article, the first being lack of complete and unbiased follow-up and the second being inability for rigorous control for confounders. These weaknesses were already identified as issues inherent to a study based on